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LEWIS'S CONCEPT OF EXPRESSIVE STATEMENTS

THE concept of "expressive statements," assertions of the mere apprehension of sense data, is fundamental to Lewis's analysis of empirical knowledge.[1] In fact, his translation of all empirical knowledge into such statements, through the interpretation of any objective judgment as equivalent to a "non-terminating" series of predictions, each of which can be formulated in "expressive" terms, is the distinctive feature of his analysis. And the importance of these "expressive statements," of course, lies in their function as a foundation—in Lewis's view, a foundation in certainty—for empirical knowledge.

Naturally enough, the feature of such statements most often challenged is their alleged certainty. On the other hand, it is also argued that, if there are any such statements at all, they are entirely trivial and play no part in knowledge. Yet Lewis's notion is a persistent one; and an attempt will here be made to interpret and support it, on the grounds that "expressive statements" are both certain and indispensable to empirical knowledge, because they are in a peculiar sense both analytic and empirical. While Lewis's logical argument in regard to them may be questionable, his empirical argument will remain unimpaired.

It must be clear at the outset that it is not primarily statements, expressive or otherwise, that are to be regarded as undeniable, but rather "presentations of sense"—"something apprehensible and statable, whose truth is determined by given experience and is not determinable in any other way."[2] And even the "statable" character of such data is only tentatively assumed here by Lewis, who later finds that they are rarely formulated, rarely need to be formulated, are "difficult—or, it might plausibly be said, impossible—to state in ordinary language," and play "the same role" whether they "could be precisely expressed, or not."[3]

It can hardly be denied that "there is such a thing as experience, the content of which we do not invent and cannot have

[1] C. I. Lewis, *An Analysis of Knowledge and Valuation* (Lasalle, Illinois, Open Court Publishing Company, 1946).

[2] *Ibid.*, p. 171.

[3] *Ibid.*, p. 182.

as we will but merely find."[4] Some attempts to deny this are really attempts to emphasize a point which Lewis asserts also—that "this is not the whole of perceptual cognition."[5] Other opposing arguments deny only the certainty or unmistakable character of this content of experience; but even here a theory of Lewis's type has an initial plausibility. On the face of it, "what we *see* when we think we see a deer but there is no deer"[6] appears to be undeniable. Whatever we may be mistaken about, the mistake, at least, is unmistakable. If we see what is regarded by some standard or other as the "wrong" thing, that is irrelevant; what we do see is certainly seen, and is the foundation of empirical knowledge. It is when we attempt to discern and specify exactly *what* is certain about experience that complications arise, providing an opening for the view that we may be uncertain or mistaken as to what we do actually experience; yet we shall find that when the complications are disentangled, Lewis's position is still intact.

To begin with, however, it must be admitted that the *logical* argument for the certainty of data, on the grounds that "If anything is to be probable, something must be certain,"[7] is inadequate, although containing an element of truth which Lewis does not quite identify. For even if the grounds of this argument are true, and certainty is required as a foundation for probability, this proves not that data are certain, but rather that valid calculation of probability is impossible, at least in any sense in which such calculation functions in empirical knowledge.

The estimate of the probability of an objective physical event (expressed by a "non-terminating judgment") is based, as Lewis shows, not directly on sense data or even on the "expressive statement" of them, but rather on predictive "terminating judgments" which, although still within the scope of data and still stated in expressive terms, cannot be so certain as mere present data or as Lewis assumes. Fulfillment of a prediction involves a period of time, and therefore memory, which Lewis admits is uncertain; for when the ostensible verification occurs, we must remember whether this is just what we initially predicted, and what the experienced grounds were on which we predicted it. This does not, however, destroy the function of terminating judgments, since, certain or not, they do "terminate"—i.e., one specified experience verifies such a judgment as completely as anything can, though not quite so completely as Lewis asserts, leaving neither the need nor the possibility of inquiring further. These judgments, therefore, pro-

[4] *Ibid.*, p. 182.
[5] *Ibid.*, p. 182.
[6] *Ibid.*, p. 183.
[7] *Ibid.*, p. 186.

vide a determinate content within direct experience for objective statements, but not a foundation in certainty.

Further uncertainty intervenes because the data, even in the form of tested terminating judgments, are not the only premises in probability inferences. First, the decision as to the relevance and adequacy of the data to be used is far too indeterminate and perhaps arbitrary (as indicated by controversies over selection of a "reference class" and by the elasticity even of Lewis's own fairly definite criteria for such selection)[8] to be considered certainly valid. Second, even in the rudimentary inference from verified to unverified terminating judgments, and again in the inference from these to non-terminating judgments, we make probability estimates whose premised data are not sense data presently apprehended. As Lewis points out, even the verifications of terminating judgments, used as the basis of subsequent inferences, appear as premises only as *remembered,* not instantaneously as they occur.[9] Moreover, such empirical inferences from them, however uncertain, must assume the empirical validity of induction.[10] And both memory and the empirical validity of induction are uncertain.[11]

Indeed, although Lewis calls his view of probability an "*a priori* theory," the certain, *a priori* element in it is the same as in the so-called "empirical theory" of Reichenbach. It is the calculation, the "*valid estimate* of a frequency," rather than the "actual objective frequency" of events,[12] that is determined by *a priori* considerations. By reserving the term "probability" for this estimate only, Lewis can regard a probability judgment as certainly valid, especially since variation in its degree of reliability is included in the judgment itself.[13] For the validity of an estimate consists of derivation from given data by "correct principles of judgment," not agreement with actual events.[14] Nevertheless, since it is the "objective frequency" of events that is to be validly estimated, and since Lewis himself emphasizes that the purpose of probability judgments is prediction,[15] he has merely postponed the question of *why* the estimate is valid, or why the "principles of judgment" are correct *for prediction.* The fundamental principle conferring *a priori* validity on probability estimates, for Lewis, is the "rule of induction"[16]; yet, in discussing actual empirical knowledge, the belief (short of certainty) that events will occur

[8] *Ibid.*, p. 298.
[9] *Ibid.*, p. 334.
[10] *Ibid.*, p. 327.
[11] *Ibid.*, p. 362.
[12] *Ibid.*, p. 303.
[13] *Ibid.*, p. 296.
[14] *Ibid.*, pp. 305–306.
[15] *Ibid.*, p. 290.
[16] *Ibid.*, pp. 272–273.

with something like the frequency estimated,[17] he assumes that this "rule of induction" will hold for *events* merely on the grounds that "we have no alternative."[18] Only in this manner can he explain why a valid probability estimate really estimates anything at all—why it indicates that an event will *probably occur*. There is no term to use here other than "probably," and Lewis offers none. But if the estimate is itself a probability, the occurrence of the objective frequency estimated must be merely "probably probable" after all.

Even the certainty of data, then, cannot provide a foundation in certainty for probability judgments, since our memory of data, the basis of their selection, and their predictive significance, all uncertain, are implied premises of such judgments. Yet this may not be quite the "disastrous conclusion"[19] which Lewis supposes. His own distinction between an estimate of a frequency and the objective frequency itself may here serve an extremely important purpose, though not quite the one for which he attempts to use it. What we really have here is not a distinction between probability and something else, but rather a distinction between the epistemological probability that our knowledge is correct and the ontological probability that an event will occur. In estimates of the probability of present or past events, the events themselves have already certainly occurred or not occurred, and we have only the problem of epistemological probability to contend with—the uncertainty of knowledge *about* the events. In prediction, however (and Lewis interprets all knowledge, at least methodologically, in terms of prediction), the matter is more complicated. On a deterministic basis, with the future regarded as "certain" (i.e., presently determinate), the uncertainty of prediction is still merely epistemological; it is already a fact that a definite event will occur, but we can know that fact only probably. If, however, we accept the indeterminism according to which the *occurrence* of any specific future event is made merely probable, not inevitable, by present conditions (i.e., even an omniscient being could make only a probable prediction about it), a prediction about it *must* be only "probably probable."

In this context, the element of truth in Lewis's initial dictum becomes clear. If anything is to be probable, something must be (not certain but) *real;* and some actual or possible statement about it must be (not certain but) *true*. There must be present circumstances which make future events probable. If there were no real world at present, nothing could happen in the future; and if the

[17] *Ibid.*, p. 323.
[18] *Ibid.*, p. 362.
[19] *Ibid.*, p. 289.

present world had no consequences which were at least partially determinate, anything could happen in the future. This is in fact what Lewis asserts when he says that there must be "real connections"[20]; and, we may add, real existents to be connected by them with those future events which are predictable. Otherwise there would be no determinate future, i.e., literally nothing to predict. Precisely *what* the situation is, however, need not be known with certainty; although the attempt to predict would be meaningless without the assumption that, whatever it may be, our knowledge has some relation to it. We must, indeed, *assume* that the information on which we base our probability estimate is true, although it need not be so in fact. In the language of Reichenbach, who is here more Kantian than Lewis although surely less willing to accept the epithet, it is a "blind posit." If our information is *not* true in fact, our estimate will probably be mistaken; but that does not prevent us from making it. Rather it encourages us to make the estimate, because the future will at least to some extent confirm or disconfirm it.[21]

On this basis, Reichenbach is right in denying that probable knowledge must, as Lewis supposes, be regarded as *certainly* probable,[22] but wrong in denying that it must, as Lewis falls just short of asserting, be regarded as probably *true* (i.e., in denying "two-valued logic").[23] On this basis, too, we are confronted with a coherence theory of *knowledge* and a correspondence or semantic theory of *truth*. Knowledge is based on assumptions and therefore uncertain, even if data should prove to be certain; and it is confirmable only by further knowledge which is itself equally uncertain. This is, indeed, recognized by Lewis in his theory of "congruence," which he rightly offers as a theory of knowledge and not of truth.[24] To break through its circularity in regard to truth, however, what we need is not the certainty of data, but the recognition that there *is* an "objective frequency" which we estimate as best we can; that *something* is objectively the case now, and something else *will* (probably) occur, which our supposed knowledge either will or will not identify.

Lewis to the contrary, then, nothing need be certain for anything to be probable. Further, if something (i.e., apprehension of data) should be in fact certain, probability judgments will not

[20] *Ibid.*, p. 227.

[21] Hans Reichenbach, *Experience and Prediction* (Chicago, University of Chicago Press, 1938), p. 400.

[22] *Ibid.*, p. 401.

[23] *Ibid.*, pp. 323, 333.

[24] Lewis, *op. cit.*, pp. 350–351.

thereby become themselves certain. Nevertheless, the alleged certainty of data, even if it lacks the particular kind of conclusive importance for objective knowledge which Lewis attributes to it, must be examined on its own merits for the importance which it does have.

The occurrence, verification, and meaning of sense data are the same. Whatever we experience *is* the data, just as we experience it. Anything outside of that experience is irrelevant to it, implies nothing about it, and therefore can neither verify nor contradict it. And it cannot itself mean (imply or presuppose or indicate or refer to) anything beyond itself, except with the aid of interpretation, which may be mistaken but is not part of the data. What, then, can be meant by those who assert that there may be conflicts or contradictions, which of course would imply errors, in our knowledge (or apprehension) of sense data?

First of all, there may be a kind of "uncertainty" that is different from the possibility of error, and might better be called "vagueness." If a sensation is faint, indistinct, changeable, or otherwise indecisive, a thoroughly correct and certain experience of it will of course experience this very indecisiveness. Next, there is the uncertainty of memory, which is involved in almost any statement we may make. Not only is there usually a time lag, however minute, between the experience and the statement; but in using any word to identify the experience, we must remember other experiences to which the word has been applied in order to recognize its applicability by comparing the present and past experiences. This brings us to the possibility of erroneous language, mentioned by Lewis.[25] It should be observed that language asserting that we see something that "seems like" some objective thing involves a dual possibility of error—not merely mistaken linguistic classification, like calling a visual impression "blue" when it is blue-green, but also the possibility that a vague appearance like that of a cloud, for instance, which "looks like" (i.e., merely suggests by vague resemblance) a camel to me, may look like something quite different to someone else. This difference is not merely one of language, but one of interpretation also.

Finally, we come to a somewhat different possibility of error. We may have an experience which, while there is no question that we have it, may be regarded, by some criterion external to it, as the wrong experience to have under given circumstances. A statement like "I thought I heard someone, but I suppose I didn't," usually means "I heard someone, but there was no one there," or

[25] *Ibid.*, p. 183.

more precisely, "I heard a sound resembling that of a footstep, but my interpretation of it as a footstep must have been mistaken, as further investigation indicates that no one was there." Most alleged cases of "conflicting data" are of this sort. So-called errors in observation, such as errors in reading thermometers, for instance, when they are not errors in recording what is seen (a form of error in language), are cases in which the wrong reading is *seen*—i.e., inattention or bias or a momentary deficiency in vision has caused the investigator to *see* something which, according to various criteria, he *should not* have seen.

These other criteria are not other data, but interpretations of other data in terms of objects; and what they disconfirm is not the occurrence of the experienced data now in question, but the interpretation which these would normally be given. In other words, although our interpretations interpret precisely those data which we do in fact experience, we adjust these interpretations so that our various items of knowledge will be consistent or coherent with each other. To the extent that knowledge includes not only the actual data but interpretation of them, a contradiction is taken to indicate that something is wrong with our knowledge; but data themselves cannot contradict each other because they assert nothing about each other.

If I read a thermometer, then, differently from ten other people, all of whom agree, this casts no doubt on the fact that I saw what I thought I saw, since "I thought I saw" has no meaning different from that of "I saw." It does disprove, however (though not with absolute certainty), that what I saw was actually the position of the mercury in the thermometer; because we assume that this actual position is indicated by the fact that each of many observers experiences data which he interprets as so indicating it. Past experience has led us to interpret our data in certain ways (i.e., to base upon it both terminating and non-terminating judgments); and, as Lewis points out, when we regard certain judgments as objectively true, we imply that other people experience data which lead them to make the same non-terminating judgment.

It would seem that any error in regard to sense data can be recognized as belonging to one of the types of error here analyzed —or at least, if the list should prove to be incomplete, to some similar type, similar in the sense that the error is involved in some manner of dealing with the data rather than in the mere apprehension of them. This means that there can be uncertainty *about* data but no uncertainty *of* data—no uncertainty or error in respect to their character at the moment when they are apprehended.

Having defined what is uncertain about data, however, we are faced with the more difficult task of defining what is certain about them. Of course, the characterization of data as "certain" can be interpreted only to mean that we have knowledge (or apprehension) of them that is certainly true. Data themselves assert nothing, and cannot be called true or false; they merely are. It is even questionable whether genuinely expressive language can be regarded as true or false. Is "Ouch!" true or false? We may perhaps regard it as true if genuinely expressive of a feeling, and false if uttered to simulate a pain which we do not in fact feel. We do not, however, exclaim "Ouch!" erroneously, unless an implicit prediction is involved; for instance, a timid patient may cry out in anticipation as the dentist seems about to pull the tooth, and then find that the actual extraction does not hurt. Something like "Ouch!" is the genuine case of Lewis's "expressive language" and fulfills his specifications perfectly (i.e., it can express a lie but not an error),[26] without the opportunities for error in interpretation involved in "I see something that looks like. . . ."

The exclamation gives expression to whatever it is that we actually feel; or pretend to feel, if it is a pretense; or expect to feel, if it is anticipatory. It is noteworthy, too, that in the case of a pretense, or lie, the falsehood is a property not of the exclamation "Ouch!" itself, which asserts nothing, but of the statement, "I have a pain," which the hearer infers from it. Neither data nor mere expression of data, then, can be true or false; and similarly, they can be neither analytic nor synthetic. Nor can they be certain or uncertain, unless we confuse these terms with distinct and vague.

Suppose, however, that the dentist challenges the "Ouch!" by insisting that "It doesn't really hurt," and the patient replies "Yes, it *does* hurt," or "I do feel a pain." Immediately the patient, who is now making an explicit statement, has an opportunity, however slight, for error—such as saying that it hurts when it merely jars, or calling something a pain which is merely a discomfort. But suppose the patient is a philosopher, and replies instead, "I am neither pretending nor anticipating; I do feel exactly what I am impelled to express by exclaiming 'Ouch!'" Surely there is no conceivable opportunity for error here, and nevertheless an assertion is now being made—a statement which, unless it is a deliberate lie, must be *true;* in Lewis's terms, an expressive statement. And here, finally, we can and must ask how and why it is certain. Is it analytic, is it a communication of something new, or can it in some sense be both?

[26] *Ibid.*, p. 179.

The occurrence of any data (which always means their occurrence as someone's experience) adds something new to knowledge because it is a new event which is, by definition, known as it occurs. Or we may state this same point by saying that to have an experience (which always means to experience data) adds to knowledge because it *is* new knowledge. If we wish to maintain Lewis's usage, which prohibits use of the term "knowledge" here, it is at least new subject matter for knowledge. The purely expressive statement that this experience has occurred must therefore communicate a new fact; it would appear that it could not be analytic.

How, then, can the statement be certain? As we have seen, it cannot be certain if it attempts to describe or formulate or even name the experienced data which it reports. Indeed, the statement "I feel what I am impelled to express by exclaiming 'Ouch!' " if we examine it carefully, does not really tell anyone just what I feel, since it is always possible that what impels me to exclaim "Ouch!" may not be exactly what impels the hearer to exclaim "Ouch!" It is indeed plausible, therefore, to interpret the statement, as is sometimes done, as equivalent to "I have the experience that I have," and hence analytic. This formulation, however, omits one important aspect of the statement. It does not mean merely "If I have an experience, it is (necessarily) the one that I have," but rather "I *do in fact* have the experience that I have," i.e., "I do in fact have an experience," or, better still, "I experience something," or "I experience x." To the hearer, x can be interpreted only as "what the speaker experiences," and the statement becomes analytic; but only because x really represents something verbally indefinable, and the substitution should not be made at all. To the speaker, however, it makes sense to substitute "I experience *this,*" or "There is *this* experience," or "this datum," where "this" can be pointed out and is undeniable.

It should be noted here that the interpretation of "Ouch!" has been used for this analysis, not because the status of pain as a datum is any different from that of visual or other sensory data, but because there is no corresponding genuinely expressive word for these in the English language. The French expression "Voilà!" is, however, comparable to "Ouch!" in expressing the data of sensory experience. It can be interpreted similarly, and perhaps even more clearly, as equivalent to "I experience x," meaning to the hearer, "I see what I am impelled to express by exclaiming 'Voilà!' " or "I see something," and meaning to the speaker, "I see *this,*" where "this" has no verbal definition but means the sense datum itself.

Certainly this is empirical knowledge if anything is; it con-

sists of having an experience and asserting that we have it. Moreover, an assertion that something exists or occurs is not ordinarily regarded as analytic. Nevertheless, the expressive statement is certain only because (and insofar as) it adds nothing to the experience but merely asserts that it occurs—that there *is* the experience (of the data which I point out to myself). It does not formulate any knowledge which adds interpretation or anything else to the event; but *the event itself adds to knowledge.* A genuinely expressive statement, therefore, cannot be regarded as either analytic or synthetic in the ordinary sense. It may be called analytic, however, in an extended sense of that term, not as an analysis of a meaning, but as an analysis of an experience, analyzing the experience into the character of the data and the fact that they do in fact occur (i.e., are experienced). Unlike the assertion that a physical object exists, which is a non-terminating judgment implying various predictions and itself inferred from various experiences, the assertion that an experience or a sense datum exists is inferred from nothing and implies nothing, but merely expresses (or points to) the occurrence of a particular experience. In this sense it is both analytic and empirical (the opposite, it would appear, of synthetic *a priori*). To the hearer, it is either a mere uninterpreted "I experience *x*," or, if interpreted as a statement, a purely analytic "I experience what I experience." To the speaker, however, it is a statement at once analytic and empirical —analytically true because it merely points to what happens without saying anything further about it, and empirical because something happens to which it can point.

The question then arises, at what point genuine communication of empirical knowledge becomes possible. We now ask, not "What do I mean?" but "What do I communicate?" What do I communicate when I say not "Ouch!" but "I feel a pain," not "Voilà!" but "I see blue"? As a bare minimum, I have compared my present datum with certain remembered data, and have found it to resemble them closely enough to warrant the use of a word for it which I have used or heard used for them. (The fact that this process usually takes place subconsciously, in the form of organic response, is irrelevant here, where we are considering its meaning, not its procedure.) How close the resemblance between the present and past instances must be if I am now to use the term "blue" will depend on the degree of precision with which I identify and remember my sensations and also the precision of my vocabulary. Moreover, in using language for communication, I assume a common and constant world, to an extent sufficient to establish a common and constant usage; i.e., I assume that the hearer has

experienced and will continue to experience data which resemble each other under circumstances at least approximating those under which I have experienced and will continue to experience data which resemble each other. Thus, in saying "I see blue," while I cannot communicate the immediately given quality of my experience, and have no way of knowing whether the hearer's experience of blue is qualitatively identical with it, I do communicate that I now see a color approximately resembling the color that I see if I look at the sky on a clear day. And unless the hearer sees approximate color resemblances among the same things where I see them, or at least among most of these things at most times, "blue" will mean nothing to him.

This means that "I see blue" is, in Lewis's terms, not an expressive statement but a terminating judgment ("I shall see a color similar to this if I look at the sky on a clear day"); and, to the extent that I expect the hearer to understand me, it involves a non-terminating judgment as well ("This color and the color of the sky are properties of similarly colored *objects,* which may be so characterized because they will also appear similarly colored to another observer, to whom I am now speaking"). Moreover, since this statement assumes that objects that were blue in the past will be blue in the future, it assumes induction, which thereby is again revealed as an extremely fundamental problem.

This leads to a further question. If expressive statements are so limited in scope, a mere "I experience *this,*" incommunicable except as an analytic statement, have they any function at all, or could they be dropped out of the scheme of knowledge without being missed? Do we actually make statements which communicate nothing determinate to others (i.e., do we speak to ourselves)? And if so, for what purpose? In reply, it still appears necessary to assert with Lewis that expressive statements, whatever their limitations, are the foundation of all empirical knowledge. It is necessary because mere experienced data, as distinguished from the assertion *that* they are experienced, cannot function in terminating judgments—which, we must remember, are still "expressive," but involve *prediction* in expressive terms. While we might perhaps manage to do without the assertion "I experience this," in view of the fact that it adds nothing to the mere apprehension of the data indicated, we could not possibly do without the assertion "I *shall* experience this." (E. g., we must have in mind the color blue as remembered, and also the prediction that we shall experience it if we look up.) Surely a significant difference between genuine cognitive prediction and mere animal expectation lies in the fact that the former can assert something about an experience

which is to occur at any future time, while the latter can merely look for something in that immediate future which is just becoming present.

Objective (non-terminating) judgments, moreover, are interpretations not of mere data (which merely are what they are and neither require nor admit of interpretation), but rather of the fact *that* these data are and others have been or will be apprehended, i.e., that they occur precisely when and as they do occur. Only if we can *assert* as well as experience the immediate, therefore, can we have any knowledge which extends beyond the immediate. This means that expressive statements are indeed indispensable. It means also that their indispensability depends not on their certainty, but rather on the immediacy of that which they assert. Of that immediacy, however, their certainty is a consequence and an assurance.

Neither certainty nor immediacy enables the sense data which are pointed out by expressive statements to fulfill a function which Lewis sometimes claims for them, that of verifying our knowledge directly in terms of an objective world existing independently of knowledge. As we have seen, it is not data, but rather objective interpretations of data, that can contradict each other and consequently can correct each other. This is indeed implied in Lewis's view that data are unmistakable. Consequently, *objective* knowledge (as distinguished from mere terminating judgments) proceeds by a mutual adjustment of *interpretations* of data; and in case of a conflict, we do not always discard prior objective interpretations, but often exclude present data instead—not denying that they are experienced, but denying that they can be interpreted as "objective" and relegating them to the status of "illusions." Data determine interpretations, but interpretations also determine the status of data. This is a circularity in which data themselves are involved, and beyond which they cannot take us. One aspect of it is revealed by Lewis himself in relying on "congruence" to decide between *memories* of data.[27]

This is, however, a circularity of knowledge and not of truth. The present *truth* of objective judgments, in Lewis's interpretation, is independent of verification. Belief in independent reality, he demonstrates by a most illuminating analysis, means empirically a belief in terminating judgments which are "verifiable but unverified," i.e., in *possible* experience,[28] with the "possibility" interpreted in terms not of material or formal implication but of a condition contrary to fact; i.e., a statement of it is determinately true or false even when the hypothesis is false (i.e., even when

[27] *Ibid.*, pp. 334, 338. [28] *Ibid.*, p. 20.

unverified).[29] But such a statement can be true *now* only if it is made true by conditions which exist now. For anything to be possible in this sense (just as for anything to be probable, and with the same epistemological implications), something must be real, and some actual or possible statement about it must be true, though not known with certainty. There must be "real connections," [30] as Lewis asserts, and also real things to be connected by them with possible consequences.

The presently existing world, then, which makes "possible experiences" possible, is known only in respect to the experiences which it makes possible. Only to that limited extent do data base our knowledge on a reality independent of knowledge. Lewis, attempting to avoid any dualistic "hiatus" or Kantian unknown "thing in itself," is able to claim for data only a numerical and not a qualitative identity with independently real things [31]—so that his attempt does not really succeed in this respect. His empirical analysis of the certainty of expressive statements, like the valid element in his logical argument for it, leads to a coherence theory of knowledge, a correspondence theory of truth, and an inevitable gap between them. But perhaps this is the best that can be done.

Nevertheless, the sense data pointed out by expressive statements do fulfill the function which Lewis *primarily* intends them to fulfill. They do provide a determinate content in experience for our objective judgments, which are themselves non-terminating and therefore at least partially indeterminate and extending beyond actual experience. It is this content that makes any judgment an instance of empirical knowledge.

BELLA K. MILMED

NEWARK, NEW JERSEY

THE HYPOTHETICAL NATURE OF HISTORICAL KNOWLEDGE

I

ETIENNE de Boré established the sugar industry in Lousiana in the latter part of the 18th century. Some years ago, the editors of the *Dictionary of American Biography* asked me to find the month and day of his death. They knew the year, 1820, and

[29] *Ibid.*, p. 226.
[30] *Ibid.*, pp. 227–228.
[31] *Ibid.*, pp. 187–188.

that he was buried in St. Louis Cemetery No. 1 in New Orleans. I went to the cemetery, but when the sexton had found the tomb, we saw that the marble headstone was badly eroded. Although we could make out that February was the month of his death, the day was irretrievably lost. It was helpful to know the month, however, and armed with this knowledge, I went to the Cathedral archives. The custodian was able to find the record of Boré's funeral. It was held on February 4, 1820. The time of day was not given, nor was the day of his death. Now knowing the closely approximate day, I went to the files of the newspapers published in New Orleans at that time. In only one of them, the *Courrier de la Louisiane,* did I find a notice of his death. The *Courrier* was published three times a week, on Monday, Wednesday, and Friday. In the issue of Friday, February 4, was the note (I translate freely), "Last night at nine o'clock, Etienne de Boré died." In the rest of the note, no further reference to the date was given. All further search for evidence was barren.

Now the question was, what did "last night" mean? The *Courrier* was a morning paper, and was on the streets by 6 a. m. In those days of hand set type and hand printing, it was most likely that the note was set up on Thursday. In that case, "last night" would have referred to Wednesday, February 2. On the other hand, at that time most funerals in New Orleans were held the day after death. When the death was as late as 9 p. m., however, and in the winter, it was not unusual to stay the funeral a day. It seemed best to decide that Boré had died on February 2.

This story gives a remarkably uncomplicated yet complete illustration of finding the facts and dating the events of the past that lie at the bottom of all historical knowledge. At the same time, it illustrates the hypothetical nature and the lack of theoretic certainty of such knowledge. Literally, no historical date is ever found, only evidence is found; and we state hypothetically whatever date best accounts for the evidence. Perhaps some day more evidence concerning Boré's death may be found through the discovery of old family records. These may establish the date with practical certainty, but theoretic certainty will still not be achieved. Up to now, however, the evidence I have related is all that can be found. For the time being, the date February 2, 1820, is official, ensconced in the *Dictionary of American Biography.*

The date February 2 was settled on by the editors of the *Dictionary* because it was the hypothesis that made all the evidence hang together best. Relative to this evidence, it was the hypothesis that was most probable. No one has any direct knowledge of the events of past history. The historian infers the account of past

events from present documents, records, accounts, physical traces, monuments, and even (though these are notoriously unreliable) memories. On the basis of these present facts, he makes a hypothesis about the occurrence of past events. That hypothesis which accounts best for all the present evidence is accepted as historical fact.

On Boré's tomb was a record which I had no reason not to assume genuine—that is, recorded by someone instructed either by an observer of the event or by someone who had reliable records. The hypothesis then was presented: if Boré died in February of 1820, the tombstone inscription is accounted for. However, finding the inscription did not establish the date of his death. To assume so would be to commit the fallacy of asserting the consequence. There are too many other possible hypotheses to explain the tombstone inscription. The hypothesis needed other verification. In addition, a complete hypothesis had not yet been stated.

The partial hypothesis helped in the search for other evidence, and soon records were found which corroborated it and at the same time made it more complete. The entry in the Cathedral archives bore out February and suggested either February 2 or February 3. It is probable that if the death had been early in the day of February 3, the funeral would have been on February 4; but it is also probable that if the death had been late in the day of February 2, the funeral would not have been held until the 4th. The newspaper said that the death was late—9 p. m.—thus strengthening the hypothesis of February 2 by giving corroborating evidence of the truth of part of it. Note, however, that the newspaper gave no direct evidence of the date. The publication was of February 4, and it said "last night"; but in the light of the conditions of printing and sale, "February 2" seemed a more reasonable interpretation of "last night" than did "February 3." Thus, the newspaper evidence made February 2 the stronger hypothesis.

At the end, a complete hypothesis was framed. I do not say that the date was found. In strict literalness, no historical date can ever be found. To speak as if it were is a figure of speech (though a quite acceptable one). Only evidence or records of the date can be found, and the evidence or records always exist in the present. Evidence is what suggests and then corroborates (or overthrows) a hypothesis. The complete hypothesis of 9 p. m., February 2, suggested by the evidence, also fitted in well with all the evidence —the tombstone inscription, the Cathedral archives, and the newspaper note.

Let me summarize the reasoning which established the date. There were three pieces of evidence. Either the hypothesis of February 2 or that of February 3 accounted for all of them. The search for other evidence was futile—no other records or monuments were found. No further corroboration of either date was found; but, as well, no other hypothesis was suggested. February 2 seemed to be a little more strongly corroborated than was February 3. No single record or piece of evidence, however, establishes a hypothesis with a high degree of probability; but the congruence of several pieces, coupled with the lack of any other strong hypothesis, may so establish it. In this case, although there was an alternate hypothesis, February 3, the congruence of all the evidence did not establish it as firmly as it established February 2. So February 2 it is—not was, but is. What we know about the date of Etienne de Boré's death is that the presently known evidence establishes the probability of the hypothesis that he died on February 2, 1820, more strongly than the hypothesis of any other date. This is the paradigm of all knowledge of past events.

II

The hypothesis of historical knowledge is not a strict deductive hypothesis. As I have stated it, it might seem to involve an inference from particular to particular; if such and such an event took place, then this record or trace is accounted for. Of course the inference here is not from particular to particular, and if it seems so, that is only because I have stated it elliptically. Generalizations, often of great complexity, are concealed in the concept of "accounted for." These are inductive generalizations, and usually are not formulated or explicit but are the precipitate of past experience, both one's own and what has been learned from others. If I had not known something of tombstone inscriptions and had not been willing to make a generalization about their reliability (barring indications to the contrary), Boré's tombstone would neither have suggested nor confirmed any hypothesis to me. A similar observation holds about the Cathedral records and about the authenticity of the newspaper. The color, texture, and brittleness of the paper upon which the newspaper was printed helped to establish its authenticity, and such assurance is based on inductive generalizations from experience. If doubt were raised by other records or traces which seemed to falsify the hypothesis based on the records that I found, and if the determination of the case were important enough, great care would have to be exercised to establish the authenticity of everything that purported to be a record.

The paper might be subjected to chemical or fluorescent analysis, the continuity of the custody of the Cathedral records and of the newspaper files might have to be established, and so on. In all of these methods of authentication, inductive generalizations would be involved.

When exhaustive authentication of records and traces is called for, all alternate hypotheses for their present existence must be presented and examined. Perhaps such hypotheses will be *prima facie* improbable, but as long as they are suggested by any evidence, or as long as there are records or traces of equal initial plausibility that can not be accounted for by the original hypothesis, they must be examined and either eliminated or established in place of the original one.

Sometimes, the authenticity of documents may be disestablished on the basis of internal evidence alone. For instance, a writing purportedly of the 4th century B.C. which makes detailed reference to events that happened centuries later or which uses language or paleography that was not developed until later is discredited. There are many other sorts of internal evidence, but in the examination of all of them, hypotheses must be set up and either established or overthrown. If the document dates from the 4th century B.C., it will not refer to events of the 4th century A.D.; but it does so refer; therefore it is not genuine. However, if the manuscript in question can be shown to be a copy made in late antiquity, then the hypothesis is presented that the references to events later than the 4th century B.C. were added by the copyist. Even though such a hypothesis is supported by only circumstantial evidence, the authenticity of at least parts of the document must be reconsidered.

It is evident that in the examination of records on the basis of internal evidence, we use the criterion of conformity or congruence with the accepted body of knowledge and accepted generalizations within such fields as history, archeology, philology, paleography, etc. In the case of the recent dating of relics by means of radioactive carbon, very elaborate generalizations of nuclear physics and plant physiology are involved.[1]

In the reasoning about the date of Boré's death, other empirical generalizations were involved in addition to those concerned with establishing the authenticity of the records. I mentioned some of these as I went along, such as the generalization concerning burial

[1] See William F. Albright, "The Dead-Sea Scrolls," in the *American Scholar*, Vol. 22 (Winter, 1952–53), especially at page 82; see also Jacquetta Hawkes, "Stonehenge," in the *Scientific American*, Vol. 188, No. 6 (June 1953), especially at page 30.

the day after death except under special conditions, and the knowledge of the circumstances under which newspapers were set up and printed in the early 19th century. All these, being inductive generalizations, have probability, not certainty. The further premise (usually implicit) that the case at issue is a case to be subsumed under the generalization also has only probability. Thus, the inference in a historical hypothesis involves a good many probabilities among the data or evidence. It is also a probability inference in another way—it bases the truth of the antecedent upon the truth of the consequences. Although this is always a fallacy in deductive or theoretically certain reasoning, it is not a fallacy in probability inference if there can be established the conditions under which the truth of the consequences makes more probable the truth of the antecedent.

III

How much reliable historical knowledge do we possess? I do not want my emphasis on the hypothetical and theoretically uncertain nature of this knowledge to suggest that we have little or none. We have a great deal of very reliable historical knowledge, and the more we have, the more we can have, for one of the ways of increasing the probability of the truth of a historical hypothesis is by establishing its congruence with other and themselves well corroborated hypotheses. If no such person as Aristotle lived in ancient Greece, there is a vast amount of presently existing data, records, and accounts for which no reasonable explanation exists. If we find reason for accepting the existence of Aristotle, that acceptance itself helps with the knowledge of the existence of Plato, for many of the records we accept when we accept the existence of Aristotle increase the probability of the hypothesis that Plato existed. When we come to Socrates, we would have little to support the hypothesis of his existence if it were not congruent with that of Plato's existence. Our assertion of Socrates' existence, considered as a separate and independent hypothesis, is on very shaky ground, yet I venture to say that not one of us seriously questions it; and the reason that we do not is that it is so fully congruent with that of Plato's existence.

The doctrine that all historical knowledge is hypothetical does not lead to historical skepticism. I say that I think we have a great deal of highly reliable historical knowledge. I do not say, however, that we have reliable knowledge of any large proportion of all that has happened to and concerning human beings in the past. Historiography is confined to the stating and investigation of

hypotheses suggested by traces, records, or accounts which are important enough to elicit our interests and be worked on. Different interests will find and concentrate on different traces and records, and will elicit different hypotheses from them. It is in this sense that Croce's dictum that history must be rewritten by each generation is true. The dictum is often interpreted to mean that history is what each generation takes it to be. If history is what happened in the past, I should suppose that it can not change from generation to generation. On the other hand, if history is our knowledge of what has happened in the past, it does change from generation to generation. The semantic confusion here would be clarified if we were more careful always to use the term "history" to refer to past events, and the term "historiography" to refer to our knowledge of past events.

The present essay, for simplicity, took the problem of dating a specific past event as its point of departure in investigating the nature of historical knowledge. It is not suggested that historiography is confined to such tasks. It is not even suggested that historiography is limited to the "facts" of history. Historiography goes on to the larger, more inclusive tasks of determining causal connections and other interrelationships between the events of history. It makes interpretations, and tries to explain human actions by speculating about motives.[2] It can not advance to any of these complications, however, without having first established some dates and reconstructed some specific events. Establishing dates may not be the most important task of historiography, but it is logically fundamental to every other task, and if the knowledge it yields is hypothetical, the hypothetical nature runs through all the further tasks. The facts are known by making and verifying hypotheses to account for the records and traces. The interpretations and speculations are hypotheses to account for the facts.

If historical knowledge is the sort of thing argued in the present essay, what can we say about the probability of being able to find a universal pattern in history? In the first place, the knowledge of such a pattern, being an empirical generalization, would itself be hypothetical. It would be hypothesis compounded, for, being based upon our knowledge of past events, it would be a hypothesis based upon hypotheses. As universal, it would be posited as the pattern of *all* past events. We know anything about only relatively few past events. Thus, the hypothesis of a par-

[2] For a discussion of these broader issues, see Edward W. Strong, "How Is Practice of History Tied to Theory?" this JOURNAL, Vol. XLVI (1949), pp. 637–644; see also Helen M. Lynd, "The Nature of Historical Objectivity," this JOURNAL, Vol. XLVII (1950), pp. 29–43.

ticular pattern would have to be extrapolated from very limited data. An extrapolation of this sort does not have much probability unless it can be supported by a crucial experiment: we surely can not experiment with past events when we can not even observe them.[3]

Such considerations as the foregoing incline me to believe that although a pattern of human history may be put forward speculatively, it can not hope to find much verification. Such speculation is useful for the insight it may yield in enabling us to interpret past currents of history. It should be remembered, however, that such interpretation is imaginative, and all its hypotheses must be verified by the historiographical process.

Thus, when I consider Hegel's or Marx's or Spengler's or Toynbee's lucubrations about the patterns of history, I am seized with intense admiration of the scope of their learning and the strength of their imaginative power to formulate hypotheses, but I see no reason for our *believing* them in such a way as to influence our plans of future action, or even our expectations. These systems of the philosophy of history are valuable to the extent that they suggest insights and interpretations; but before they can be considered knowledge, they must be verified by reference to empirical historiography. The material for their verification is both slight and hypothetical.

HAROLD N. LEE

NEWCOMB COLLEGE, TULANE UNIVERSITY

COMMENTS AND CRITICISM

NATURALISM AND THE METHOD OF *VERSTEHEN*

In a recent discussion in this JOURNAL,[1] Miss Thelma Z. Lavine has argued that contemporary naturalists have generally failed to make the crucial distinction "between the method stipulated by naturalism for inquiry . . . and the method of naturalism itself."[2] Consequently, she has contended that naturalism has likewise failed "to raise the question of its own method as distinguished from that of science."[3]

[3] See the present author's essay, "A Criticism of the Marxian Interpretation of History," in *Tulane Studies in Philosophy*, Vol. I (1952), p. 95.

[1] "Note to Naturalists on the Human Spirit" and also "What Is the Method of Naturalism?" this JOURNAL, Vol. L, No. 5, Feb. 26, 1953, pp. 145–154 and 157–161.

[2] *Ibid.*, p. 146.

[3] *Ibid.*, p. 148.

Miss Lavine sees, as a result of these failures, an apparent incompatibility between what the naturalists do, and what they say they do; and she concludes that the true method of naturalism ought to be—and in fact is—not the scientific method, but what she calls the "method of *Verstehen.*" This method, Miss Lavine suggests, not only releases naturalism from several "self-imposed restrictions,"[4] but also constitutes in fact "the sole method of philosophy."[5] It is to these remarks that I wish to reply here.

That naturalists have failed to distinguish between the method stipulated by naturalism for inquiry into all subject matter, and the method of naturalism itself, is not at all surprising. The reason for this "failure" is simply that this is a distinction which naturalists deliberately refuse to make. They have been keenly aware—as Professor Dewey so often remarked—that philosophy is one human concern among many, and though it may have aspects and characteristics peculiar to itself, one of these is not exemption from experimental inquiry. It is not, then, entirely fair to say that naturalists have failed to raise clearly the question, *"What is the method of naturalism?"*[6] What they have failed to do—or rather, what they have intentionally not done—is to separate this question from the more basic question of which it is a part, *"What is the method whereby reliable knowledge may be obtained, if it is obtainable at all?"* To this question the naturalist replies: the scientific method, the experimental method, the method of inquiry.

Of course the applicability of scientific methods in philosophy has long been a matter of dispute; and one of the major causes of disagreement upon this question has been the confusion which has arisen over the terms "scientific method," "experimental method," and "method of inquiry." Naturalists, who are generally most insistent upon the unrestricted application of such methods, are keenly aware that what they do consists of much more than just "making experiments"; yet they wish also to insist that the methods they employ are scientific and, in that sense, "experimental."

Understandably, this has led to considerable confusion, for it is clear that these terms have no universally accepted significance. Taking them, apparently, in a rather narrow sense, Miss Lavine concludes that there must be some other methodological element which allows naturalistic philosophers to do as they do. To supply this element, she suggests the acceptance of the method of *Verstehen.*

[4] *Ibid.*, p. 154.
[5] *Ibid.*, p. 153.
[6] *Ibid.*, p. 147.

In appraising this notion of *Verstehen,* two questions must be asked:

(1) Is it the case that the experimental method, properly taken, is inadequate to account for the method of naturalists as naturalists?

(2) If this is the case, is the replacement of the experimental method by the method of *Verstehen* sufficient to account for the method of naturalists as naturalists?

To answer these questions it is necessary to refer both to the first and second of Miss Lavine's papers, and to Mr. Nagel's [7] reply to the former.

(1) In discussing the principle of continuity of analysis, Mr. Nagel remarks that the adoption of it is no obstruction to the acceptance of any of the conclusions to which an experimental method may lead. He then goes on to point out that naturalists do in fact "subscribe to quite a variety of fairly general conclusions which can count as components in a constructive philosophy." [8] But Miss Lavine assumes that these general conclusions must have been got in some way other than by means of controlled inquiry. She writes, "Here it appears that Mr. Nagel is discussing a second order of conclusions, not those of the method of experiment and verification but 'fairly general conclusions' which are presumably derived in some way from the conclusions of scientific method." [9] Now, that these general conclusions are derived from the conclusions of scientific method, and that they are in that sense of a "second order" is certainly the case. But it by no means follows that, because these are derived from the conclusions of scientific method, they are therefore not derived *by* the scientific method. That is to say, because these general conclusions are of a second *order,* it does not follow that they must have been derived by a second and different *method.* It is just here that Miss Lavine's conception of the scientific method appears overly limited. It is not only the case that we apply experimental methods to the stuff we put in test-tubes, but also that we apply such methods to the conclusions we draw from test-tubes; and further, we apply experimental methods to the conclusions we draw from these conclusions. This is precisely why the naturalist refuses to distinguish between the method *of* naturalism, and the method stipulated *by* naturalism for all inquiry. There is no real distinction to be made.

[7] "On the Method of *Verstehen* as the Sole Method of Philosophy," this JOURNAL, Vol. L, No. 5, Feb. 26, 1953, pp. 154–157.

[8] *Ibid.,* p. 155.

[9] Lavine, *loc. cit.,* p. 158.

However, neglecting this further application of the principle of continuity of analysis, Miss Lavine writes, "It appears, then, that many diverse operations must be performed upon the conclusions of the method of experiment and empirical verification in the construction of a naturalistic philosophy. The question, therefore, finally emerges: What is the nature of these operations? What method does the naturalist bring to bear upon the conclusions of science?"[10] Miss Lavine is quite correct when she points out that "these and no other types of operation constitute the functions of the naturalist"[11] and the naturalist replies that the nature of these operations is controlled inquiry, that the method which he brings to bear is the method (but not necessarily the exact procedures) of science. For the naturalist is very much aware of the fact that synthesis, interpretation, and imagination are as vital to the scientist as to the philosopher, and that neither science nor philosophy is possible without such activity. The point that the naturalist insists upon is that synthetic and interpretive functions are not beyond the scope of the scientific method, but constitute, in fact, a vital part of it. The answer to question (1), then, is, "No, the method of controlled inquiry is not inadequate to account for the naturalist's philosophic activity."

(2) In the light of this conclusion, the question "Does the method of *Verstehen* provide a remedy for the difficulties which the scientific method presents?" cannot reasonably be asked. But, if we assume for the sake of argument that the scientific method is somehow inadequate, we shall find that what Miss Lavine presents as the method of *Verstehen* is equally so.

It is difficult to say exactly what Miss Lavine intends by the "method of *Verstehen*"; nowhere in the two articles is she explicit as to its nature. But whatever we may take it to be in the light of Miss Lavine's discussion, we will see that it either: (a) suffers from the same restrictions imposed by the necessity of empirical verification as does the method of inquiry, and hence cannot remedy any of the latter's defects; or (b) it will not be an adequate account of the naturalist's methods. To be sure, this is but another way of saying that the method of inquiry is the naturalist's method; but we can now reach this conclusion from a different direction. For it sometimes appears, in Miss Lavine's account, that she means by the method of *Verstehen* nothing more than is ordinarily meant by the experimental method in philosophy. Thus she speaks of Dewey's discussion of the "empirical method in philosophizing."[12]

[10] *Ibid.*, p. 158. [11] *Ibid.*, p. 158.

[12] Lavine, *loc. cit.*, p. 152. Miss Lavine has reference here to *Experience and Nature*, second edition (Chicago, Open Court, 1929), Chapter 1.

which he surely did not intend to distinguish from, or oppose to, the empirical method in all other human affairs. Thus also, she writes of a "naturalistically reconstructed method of *Verstehen*" in which "the most important single task is working out a set of controls for the *verstehendes* element in philosophic and scientific theory which will serve, as do the controls in experimentation, as empirical checks."[13] But this is most misleading. Such empirical controls—in philosophy as elsewhere—are neither more nor less than what naturalists have insisted upon in their application of scientific method. It would seem that these are but two names for the same method.

In other passages, however, it appears that the method of *Verstehen* is not only different from the method of verification and experiment, but different as well from any method which most naturalists employ. Thus Miss Lavine writes that "*Verstehen* is the sole method of philosophy,"[14] and that hence all philosophies must in fact employ it. If this is the case, not only can it not characterize naturalism, but if it is held in common by naturalists, absolute idealists, neo-Thomists, and materialists, and so forth, it must be a notion of such a general nature that it could have no significant similarity to the specific methods which the naturalist does in fact employ. This will be discussed again in the concluding paragraphs of this paper. Again, the method of *Verstehen* is said to "have the function of interpreting meaning,"[15] that is, the interpretation of motivation in concrete human action, and the interpretation of symbolic action, etc. If this is to be taken as the formulation of interpretive hypotheses subject to experimental control, what is it more than the scientific approach as generally understood? If, on the other hand, this is to be taken as interpretation exempt from controlled experiment, it is a method which naturalists would specifically reject.

One may conclude that the attempt to distinguish the method *of* naturalism from the method stipulated *by* naturalism for all inquiry, and, in the case of the former, to replace the experimental method by some "method of *Verstehen*" results from either: (a) a misunderstanding of what the naturalist is actually driving at—an error which Miss Lavine is clearly not making; or (b) an overly narrow conception of what is involved in and encompassed by the canons of the "scientific method"—or what Dewey calls "the method of intelligence." This does in fact seem to be the issue in the present case.

[13] *Loc. cit.*, p. 152.
[14] *Ibid.*, p. 153.
[15] *Ibid.*, p. 150.

Finally, two brief comments seem to be called for.

(1) Miss Lavine makes the surprising claim that "Alternative philosophies cannot differ in the methods they employ, but only in the types of terms they select as the objects of reflection."[16] I am at a loss to determine what Miss Lavine means by this. It would appear obvious that philosophies have differed and do differ greatly in the methods they employ. The only sense in which the claim may be granted is that in which the "method" of philosophies is taken to signify no more than "that they involve thinking," or perhaps just "thinking" in some vague general sense. Since it will be admitted that philosophies do all involve something of this sort, if one argues on this basis that their methods "cannot differ," the statement, as true, is trivially satisfied.

(2) Because naturalists generally hold to the principles of scientific inquiry, and the continuity of analysis, it must not be concluded—as some have concluded[17]—that all non-scientific concerns are, for the naturalist, neglected, discarded, and washed down the drain. Quite to the contrary, these principles apply to the domain of cognitive affairs, to the instrumental, to the quest for reliable knowledge. But, as the naturalist is anxious to point out, it is clear that the activity of men is not wholly cognitive, and thus it is equally clear that these methodological principles do not regulate the whole of human experience—though it is the naturalist's contention that there is no aspect of that experience within which the method of inquiry is *inherently* incapable of being applied.

CARL COHEN

UNIVERSITY OF CALIFORNIA,
LOS ANGELES

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[16] *Ibid.*, p. 153.

[17] W. H. Sheldon, "Critique of Naturalism," this JOURNAL, Vol. XLII, May 10, 1945, pp. 253–270. See also, "Are Naturalists Materialists?" this JOURNAL, Vol. XLIII, April 11, 1946, pp. 197–209.

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NOTES AND NEWS

C. D. Broad, Emeritus Professor of Moral Philosophy in the University of Cambridge, is at the present time Flint Professor of Philosophy at the University of California in Los Angeles. He is giving courses in British Empiricism and Ethical Theory.

In the fall of 1954 the Flint Professor of Philosophy will be W. T. Stace, Professor of Philosophy in Princeton University. Professor Stace will give a course on Metaphysics and a seminar on Theory of Knowledge.

Professor Rudolf Carnap has accepted a permanent appointment as Professor of Philosophy at the University of California in Los Angeles beginning with the academic year 1954–55. He will give seminars and guide the work of advanced students who wish to proceed to the doctorate.

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